June 30, 1953

Dear Spicer:

I don't know whether my note2of the 18th April was a proper reply to yours of the 23d, or whether I had thanked you for the H serums received some long time ago. If not, let the present suffice.

Nothing very spectaculab has happened during the last few months. The weather is getting b'!y hot, of course, but we'll have to survive it. We did not get to the CSH symposium, but are expected a visit from Hayes later on. Tom Nelson and I have been studying elimination patterns more closely, in diploids from Het F+ x F-, and from diploid F+ x haploid F-. Along the lines of our discussions here, the results seem to require that the elimination be from a complete diploid, after there has been an opportunity for crossing-over. E.G., from Mal+S^{s} F+ x Mal-S^{r}F-, the diploids (selected as phototrophic, Lac+/- etc) are about 85% Mal-S^{r}, 12% Mal+S^{s}, 3% Mal+S^{r}, but none of them diploid for Mal or S. The latter items are inconsistent with the pre-elimination of Mal-S from some of the F+ gametes.

Not a thing on somatic antigen transduction, except some fortuitous possibilities. In one case (not many trials) TM—x abortus equi gave a IV V XII a:enx. Edwards is trying to convince me that this has to be a coincidental transduction of V and "diphasicity"; selection was only in enx motility agar, but the evidence that this is a transduction of either character is not yet very strong. The enx serum (from ab.-equi, isn't it, might have had some IV XII) or there may have been more subtle selection for a spontaneous Form Variation. There have also been some losses and gains of I in flagellar transductions to paraA and durazzo, but these are of even less certain significance (though Kauffmann seems to be convinced by one of his own examples). Have you seen some papers by Iseki (Proc. Japan Academy 1953) on E1-E2 transformations? Some Japanse had tried to repeat Edwards and Bruner's expts., using boiled cells for absorbing the sera (as we discussed on the highway to New Orleans), and could get E2 to E1, but not the converse. On the other hand, E2 is supposed to have a phage that converts E1 to E2 by infection (all the time, not as in transduction). Edwards is digging out his old intertransformed anatum—cambridge, etc., and will I hope check on this. You may have seen by now a ms. by Edwards and myself on the flagellar transductions— he sentbone to Joan Taylor.

Are you still interested in George Boole? The Dover edition of Laws of Thought is available (hardcover, at $4.50 less 10%)— would it help you if I sent it? How are you making out to get me a Weatherburn?

Still begging, I wonder if you have gotten round to making those IV and V sera you once mentioned, and if so could I prevail on your generosity for some (to use in cleaning up the ab-equi, which is rather messy now. Edwards has gone over the Hunzendorf story, and thinks c' is the C1 somatic antigen (as everyone else suspected). I haven't checked on the thermostability which was my only evidence against. Anti-C1 is remarkably effective in inhibiting motility if this is so, and it might make a good system if we could do any transductions in it. So far, no. Can you enlighten me as yet on the dublin cultures, and especially the mutants you left behind (PC-1,-2,-3;AT-2,8,14) or should I chuck them out?

One last favor: do you think you could smuggle out a photo of your lab. colleagues? I'd especially like to see what Felix and J.T. look like.

The most interesting experiments around here in a long time are in near prospect. We've finally been able to work up some Gal-/Gal-, Lp^{s}/Lp^{s}, Gal+/Gal-, etc., diploids for miscellaneous expts. on the localization of lambda-infection, Gal-transduction, etc. Skaar's expts. on motility/F are rather up in the air. Motility itself seems to have nothing to do with it, and the procedure may well be just a roundabout means of quaran-

The results are not nearly as consistent as we'd like. Some of the F- are (at least not readily) not reinfectable, but we have too few F- from other sources to have any real basis for comparison. Skaar is continuing with it, would probably prefer not to talk too loudly about it until it is somewhat less confused.

How is your health— are you well over your Brucella? How did it ever happen? We are, I must admit, slightly concerned not to hear more directly from you.

We are looking forward to see Bernstein here after a while. I hope he will have a chance to visit you before he leaves. He is rather concerned about dollar problems; I thought you might be able to advise (or at least reassure) him

He is the only prospective addition to the lab. Tom will be staying on another year, possibly two, as he has won a National Cancer Institute (sic!) fellowship. Larry is also, rightly, pleased to have won a National Science Foundation Fellowship, which makes him much better off. Dave's plans are not settled. We are still hoping for remodelling to improve our space; we have occupied all the space inside the door at the landing, but it will take some work to improve it to full efficiency. If you should meet any other candidates for a temporary emigration, let us know, as we'll probably be able to accomodate some next year (54-55), or perhaps sooner.

Do you remember Boris Rotman (Spiegelman's former student). He's coming up as a post-doctoral fellow to work (mostly at the Enzyme Institute) on what is involved in the "activation" of lactase when E. coli cells are broken.